the method they had adopted for measuring by a watt meter the total losses due both to hysteresis and eddy currents.

Dr. W. E. Sumpner and Mr. R. W. Weekes, in a paper on the Hopkinson test as applied to induction motors, stated that they had found Dr. Hopkinson's well known method, with certain modifications, so satisfactory in testing induction motors that they considered it was desirable to direct attention to the experimental details and to give particulars of the arrangements of the machines and the instruments needed for the test, and of their methods of determining the belt losses. In an appendix the results obtained in testing three-phase motors by this method were given.

Dr. W. M. Thornton contributed a paper on distribution of magnetic induction in multipolar armatures. The object of this paper was to discuss the best methods for determining the most suitable radial depth of an armature core for either continuous or alternating machines.

The last two papers dealt with on August 22 had reference to standards of light. Prof. J. A. Fleming, in his paper entitled "Large Bulb Incandescent Electric Lamps as Secondary Standards of Light," described a method he had devised of using as a secondary standard of light an incandescent filament fixed in a large bulb about 12 centimetres in diameter and about 20 centimetres long. The use of this large bulb diminished considerably the deposit of carbon upon the interior of the glass, and therefore the rapid falling off of candle-power with age of the lamp. He had found such lamps very suitable for photometric purposes. His method of employing them was equivalent to the well known system of double weighing. One of these standard lamps was balanced in the photometric gallery by an ordinary incandescent lamp, and then the standard lamp was removed and the lamp the candle-power of which it was desired to determine was substituted for it, and moved in or out towards the photometer disc until it also balanced the ordinary incandescent lamp previously used against the standard lamp. The second of the two papers was by Mr. Clifford Paterson, and was entiled "Some Investigations on the Ten Candle-power Harcourt Pentane Lamp made at the National Physical Laboratory." The author has been carrying out an investigation at the National Physical Laboratory on the effect of changes in the barometric pressure and of moisture in the atmosphere on the flame standard, and the work is still going on.

On Tuesday, August 23, the first business was the reading of the report of the Committee on the Mersey Tidal Regime. Briefly the committee has found that though extensive dredging has now been carried on in the Mersey for such a long period of time, the regime of the tides has hardly been altered.

Major Sir Hanbury Brown then read a most interesting communication on the control of the Nile, the paper being fully illustrated with lantern slides. The author described the whole of the engineering works which have been constructed for the purpose of increasing the area of irrigation in Egypt since the days of Arabi Pasha's abortive revolution. Two great schemes have now been completed, first, the reconstruction of the old delta barrage so that it now can be completely utilised for its original purpose and a head of 20 feet of water can be held up, and, secondly, the construction of the great Assuan dam and Assiut barrage. The wonderful increase in the industrial prosperity of Egypt which has been brought about by these great engineering works reads almost like a fairy-tale, and not the least important of the social advantages has been the abolition of forced labour or corvée. In the discussion, Sir Colin Scott-Moncrieff, who has been responsible for so much of this splendid work, told an amusing story of how, during the reconstruction of the great delta barrage, the last hole, which had almost baffled the engineer, was at length closed by the use of costly curtains taken from an unused Khedivial palace standing near the barrage.

Mr. J. H. Wicksteed's paper on a universal testing machine of 300 tons for full sized members of structures described the machine made for the French Government by his firm, Messrs. Buckton and Co., of Leeds, which was officially inaugurated at the Conservatoire Nationale des Arts et Métiers on June 16 of this year. This formidable machine will take in columns or tension bars 88 feet long by 3 feet 3 inches by 3 feet 3 inches, and it will admit beams 3 feet 3 inches broad by 6 feet 6 inches deep on a 20 feet span, and can be changed from one form of test to another with great rapidity. On the day of the inauguration, a steel tension bar was broken under a load of 228 tons, and immediately afterwards a thick slab of armoured concrete was broken on a span of 16 feet 6 inches.

Prof. J. O. Arnold then read a paper on the fracture of structural steel under alternating stresses, in which he described a method he had devised and the special machine he had invented for placing test specimens under severe alternating stresses slightly beyond the elastic limit; his method, as it were, carried out the Wöhler tests in two or three minutes. Though the investigation was still going on, the author stated that the results obtained so far convinced him that the micrographic methods of examining steel had in their turn failed to show the cause and prevention of sudden rupture under vibration and alternation of stress.

The last two papers on August 23 were contributed by Mr. R. A. Hadfield. The first was on the production of magnetic alloys from non-magnetic metals. In it he stated that Dr. Heusler had produced a magnetic alloy of copper, aluminium, and manganese, and that the magnetic properties of this alloy were to be attributed entirely to the presence of the manganese. Mr. Hadfield himself had made the alloy, and was still carrying on an investigation in reference to it. The alloy was very brittle, and could not be forged either cold or hot. The second paper, on experiments relating to the effect upon the mechanical properties of iron and alloys of iron produced at liquid air temperatures, gave a brief account of some interesting experiments on the effect upon pure iron of great cold. Much fuller details of this investigation are to be given at the forthcoming meeting of the Iron and Steel Institute.

On Wednesday, August 24, the last day of the meeting, several papers of interest were taken, the first being a paper by Mr. Horace Darwin and Mr. C. V. Burton on side-slip in motor cars. The results which had been obtained by the authors in their investigations into this question were illustrated by a small model car, which ran down an inclined plane. The authors were of opinion from these experiments that side-slip would be considerably reduced by steering with the hind wheels and driving with the front wheels, though this car would not be so convenient for steering as the ordinary car.

Prof. Ernest Wilson gave a further account of his experiments on the electrical conductivity of certain aluminium alloys as affected by exposure to London atmosphere. The author has recently been employing micrographic methods in his investigations. He was of opinion that the great difference which existed between the tensile strengths and the other qualities of these alloys was not due to variation in structure.

Mr. J. Casey then described the proposed barrage of the River Thames, and two brief papers, one on the testing of alternating current motors by continuous current, by Mr. Wm. Cramp, and the other on the action of lightning strokes on buildings, by Mr. Killingworth Hedges, brought the proceedings of this section to a close.

It is pleasant to record that the attendance at Section G at Cambridge was considerably above the average, and this was all the more gratifying inasmuch as several of the papers contributed were of unusual importance and of high scientific value. T. H. B.

PHYSIOLOGY AT THE BRITISH ASSOCIATION.

THE president, Prof. C. S. Sherrington, F.R.S., delivered an address on the morning of August 18, choosing for his subject the "Correlation of Reflexes and the Principle of the Common Path." This address has already appeared in NATURE (September 8).

Prof. J. A. MacWilliam read a paper on reflex and direct muscular response to galvanic currents in fishes. His experiments had proved that eels were remarkably responsive to electrical currents, a responsive fin movement of a reflex nature being readily elicited. The negative pole was usually the effective one. Frogs, newts, carp, &c., gave negative results. After death of the spinal cord much stronger currents were necessary to evoke any movement, and these were of a different character, being direct responses of the muscles.

Prof. W. H. Thompson read a paper on the metabolism of arginine. If arginine, an important crystalline base obtained by the cleavage of proteids, is administered to animals either by injection or with the food, from 80 per cent. to 90 per cent. of its nitrogen is excreted as urea. In the laboratory only 50 per cent. of the nitrogen can be split off from arginine as urea, the remainder appearing as ornithin. Hence in the body the ornithin nitrogen is also converted into urea, largely or entirely.

Prof. A. Kossel (Heidelberg), referring to the structural formula of arginine, pointed out how half of its nitrogen could be split off as urea by a simple hydrolysis, while the remaining moiety required oxidation before it could be obtained in this form. Dakin and he had shown recently that the liver and some other tissues produced a ferment capable of effecting this hydrolysis. Moreover, they had also found another ferment which possessed the power of acting upon this base in an exactly similar manner while it was still combined within the molecule of certain of the simple proteids (the protamines). The significance of these facts is of great importance in view of the wide distribution of arginine throughout the cell proteids of the body.

Dr. F. G. Hopkins stated that he had some time previously himself taken by the mouth a mixture of the hexone bases, and subsequently watched the hourly elimination of urea. He found that the rate of excretion of urea showed two maxima, the first of which he had ascribed to that part of the N of these bodies which could be obtained in the laboratory as urea, and the second maximum to the remaining N (ornithin, &c.).

Prof. E. H. Starling read a paper on the relation of trypsinogen to trypsin. Pawlow and his pupils have shown that fresh pancreatic juice, obtained from a pancreatic fistula, possesses no power of digesting proteids, but that after it has been acted upon by intestinal juice it gains that power. He concluded that the intestinal juice contained a ferment (enterokinase) which acted upon the trypsinogen of the fresh pancreatic juice, converting it into trypsin. Against this view French observers have brought forward another, viz. that the interaction of the two secretions is analogous to that of the cytases, and that the trypsinogen can only act upon proteids in the presence of enterokinase. Bayliss and Starling have studied the action of enterokinase upon trypsinogen, and by observing the rate of its action have, by finding that it follows the usual laws of ferment action, brought strong evidence to prove that Pawlow's view is the correct one, and that enterokinase is a "ferment of ferments." They have now further evidence in the same direction. By injecting rabbits with solutions of enterokinase, they found that an antibody could be produced which, acting upon enterokinase, was able to inhibit its action upon trypsinogen. Although this in itself could not be regarded as definite proof because the facts might bear another interpretation, yet taken in conjunction with the former evidence it was confirmatory of their view.

Dr. F. A. Grünbaum pointed out that the last evidence brought forward by Prof. Starling did not disprove the view that enterokinase and trypsinogen acted in a complementary manner to one another, since the result might be readily explained on the assumption that the antibody possessed a relatively greater attraction to the enterokinase than to trypsinogen.

Dr. W. E. Dixon communicated the results of his experiments upon the action of alcohol upon the heart and circulation. He pointed out that much of the literature upon the subject was valueless because the experiments had been conducted upon animals already under the influence of anæsthetics. The previous administration of chloroform or ether entirely abolished the first effects of alcohol. The experiments must therefore be conducted upon unanæsthetised animals or upon surviving organs. He proved that the first effect of alcohol upon the pulse was a slight acceleration, which he thought was due to an irritative effect of peripheral origin. The first action upon the heart was distinctly a stimulating one, as proved by cardiometer experiments. The effect upon the peripheral blood-vessels was a dilatation of the limb vessels associated with a constriction of the vessels of the splanchnic area. The effect upon the blood-pressure was a preliminary rise which was only converted into a fall when considerable doses had been given. If larger doses were suddenly administered the effect upon the heart was usually marked inhibition, which he ascribed to a direct action of the drug upon the cardiac centre.

Prof. E. A. Schäfer remarked that in some recent experiments he had found that if a mixture of chloroform and alcohol was made to replace the chloroform with which the animal was being anæsthetised, the result was an acceleration of the heart-beat, together with an increase in its force. This was not due merely to the diminution in the amount of chloroform administered.

The morning of August 19 was devoted to a discussion upon the relation of oxidation to functional activity. A report of this discussion appears in another part of NATURE (p. 590). Mr. Hankin's remarks on the spread of plague will be printed separately in another issue.

Dr. W. H. R. Rivers communicated the results of some of his observations on the senses of the Todas. These were made by methods similar to those which had been employed in the work of the Cambridge Anthropological Expedition to the Torres Straits, and the results were in general confirmatory of those reached by that expedition. The observations on Papuan and Toda seem to show that there is no marked difference between uncivilised and civilised races in purely sensory powers. Any superiority in the sensory and perceptual feats of the savage is probably due to his powers of observation and of drawing inferences based on his familiarity with his surroundings. Where there are differences between Papuan, Toda, and European, the Toda occupies in general an intermediate position between the Papuan and European, just as he occupies an intermediate position between them in intellectual and cultural development.

The only striking feature which marks off the Toda from the others is the great frequency of colour-blindness. Whereas this condition is absent or very rare in some savage races, the proportion of colour-blind persons amounts to 12·8 per cent. in Toda males as compared with about 4 per cent. in European races.

Dr. C. S. Myers communicated a paper upon recent developments in Helmholtz's theory of hearing. He alluded in the first place to Ebbinghaus's conception of an internodal vibration of the basilar fibres, and showed its value in providing a theoretical basis for the degree of relationship between the various musical intervals. Next he referred to the discovery of intertones (Zwischentöne) by Stumpf, and to their importance in determining the number of adjacent basilar fibres thrown into vibration by any simple tone, and in modifying the principle of specific nervous energy as applied to the ear. Schäfer's theory of the origin of subjective combination-tones was then described, and the difference between objective and subjective combination-tones was discussed. Lastly, Dr. Myers showed the great value of Helmholtz's theory in best explaining the known pathological phenomena of hearing, and suggested that the hair-cells rather than the basilar fibres might be the sympathetically vibrating end-organs. Such a modification involved the application of altered physical considerations to the organ of Corti, but appeared more rational and less difficult on the whole.

In the morning of Monday, August 22, the work of the section was devoted to a discussion on conduction and structure in the nerve-arc and nerve cell.

Prof. J. N. Langley, in opening this discussion, said that he restricted himself to a consideration of the general scheme of structure and arrangement of the nervous system in vertebrates, and the broad relation of this scheme to nervous functions. At present there are two main ideas of structure, one often called the neurone theory, according to which the nervous system is made up of a multitude of neurones or cells which have no connection with one another, and the fibrillar theory, according to which the nervous conducting part consists of minute fibrils joined together here and there into a network. Prof. Langley argued that whatever view is taken of the structure of the nervous system, the facts of degeneration of nerves show that it is made up of a number of trophic units, and that the theory of trophic units held whether the unit consisted of one or of a hundred cells, and whether the units were in continuity with one another or only in contiguity.

A second point which seemed certain was that the properties of the central nervous system required for their explanation some structure not present in the peripheral nerves. This structure might be, in part, the nerve-endings of the trophic units, but, in part, it must be referred to the nerve-cells, which, in fact, consisted of different protoplasm from that of either nerve fibres or nerve-endings.

If the fibrillar theory were true, there were facts which showed that the fibrils must be different in different parts of their course. This was illustrated by the action of nicotine and of other poisons on the different parts of the nervous system. With this modification, the fibrillar theory simply transferred to a part of the cell functions which were commonly supposed to belong to the whole. But it could not be regarded as certain that there were any fibrils at all in the nerve cell, for the microscopic appearances varied considerably according to the method of preparation.

A point which was much contested was the question whether the trophic units were continuous with one another or not. This point was not of great physiological importance, but the physiological facts were best explained on the assumption that the units were contiguous but not continuous.

The last point considered was whether the unit consisted of a single cell or of many cells. The study of the development of nerves had led different observers to entirely opposite conclusions. Experimentally, the question was of interest in connection with the regeneration of nerves. Numerous surgeons had found new nerve fibres in the peripheral ends of cut nerves, but their observations failed to show that some central connection had not been established. In some recent experiments made by Prof. Langley in conjunction with Dr. Anderson, it was found that without a single exception, the new fibres had become connected with the central nervous system. The balance of evidence was then against the occurrence of autogenic regeneration and in favour of the unit consisting of a single cell.

Dr. A. Hill stated that he was entirely prepared to give his approval to the neurone theory as defined by Prof. Langley, but he objected that this was merely a statement of the cell theory and did not require the special title given to it by Waldeyer. He was inclined to think that the more light we gained on this subject the more should we find that Bethe's view was correct. Apathy has shown a network of neuro-fibrillæ in nerve cells of invertebrates. This network is easily shown, and is beyond all doubt a structure existing during life. In the spinal ganglion cells of vertebrates a somewhat similar appearance is obtainable. It was easy to make preparations of vertebrate nerve cells in which fibrillæ were indisputably present, but how far this appearance was due to reagents it was impossible to say; but there was a strong probability that the net arranged itself about an existing system of fibrils. The connecting link appeared to him to be the "thorns," and it was a remarkable fact that the spacing of the thorns corresponded to the spacing of the pericellular network. Far as we were from being in a position to form a conclusion on this subject, it was not impossible that neuro-fibrillæ, Golgi's net, and thorns form a system of conducting fibrils of extreme tenuity and almost infinite complexity.

Prof. Graham Kerr gave an account of the results of his researches on the development of the nerves in Lepidosiren. His first studies on the mode of growth of the nerves in these animals seemed all in favour of His's view that the nerves developed as outgrowths from the spinal system, but more extended observations upon embryos in various stages of development led him to the conclusion that the fibres originated as strands of undifferentiated protoplasm extending between the neuroblasts of the spinal cord on the one hand and the developing myotonic cells on the other. At a somewhat later stage fibrillæ appeared in these strands, and still later a sheath was formed from mesoblastic tissue which surrounded and enclosed the group of fibrillæ. A more doubtful conclusion which might perhaps be drawn was that the original path was one along which impulses surged to and fro, and that consequent upon this use the fibrillary structure was developed as a more convenient substratum for the maintenance and extension of that function.

Dr. Mann pointed out that nerve cells might be theoretically in one of three states, viz. separate units, or continuous with one another, or at one time continuous and at another separate. In all embryos at a certain period the motor cells in the cord form a syncytium with scattered nuclei, an arrangement which later on becomes less and less marked until in most cases the cells form separate units. Cells not derived from a common mother cell are never in continuity. He pointed out that great care was needed in drawing conclusions from any preparations where such electrolytes as corrosive sublimate were used for purposes of fixation, inasmuch as all coagulation by electrolytes invariably leads to a very distinct fibrillar appearance. This is much less marked after the use of such non-electrolytes as osmium tetroxide or formaldehyde free from formic acid. At present, therefore, we are not in a position to make any assertions as to the existence or non-existence of fibrils in nerve cells or in tissues.

Dr. W. B. Hardy also directed attention to the treacherous nature of the evidence of fibres and networks in cells. A fibrillar structure can be produced from a perfectly homogeneous solution of egg-white by fixing it with the ordinary reagents and staining it in the usual way. Again, if a concentrated viscous solution of egg-white be stretched between two points and then treated with the ordinary fixing reagents, it can be shown that the fibrils produced in it run longitudinally, and are connected by less prominent ones which run transversely. These fibrils must, of course, be purely artificial.

Dr. H. K. Anderson emphasised the point that though the neurones might be physically continuous, yet on the whole they must be trophically discontinuous. Experimenting upon very young animals, he had found that section of a post-ganglionic segment led to degenerative changes in the corresponding preganglionic segment. On the other hand, the converse was not true. As a further point against the view that the fibrillæ of a preganglionic segment were continued down into the postganglionic fibres, he pointed out that Langley had shown that the mode of termination of the preganglionic fibres in the sympathetic ganglia was not specific, since an ordinary motor nerve can be made to grow down to a sympathetic ganglion, and terminating there in its own specific manner could yet establish physiological continuity.

Dr. E. Overton pointed out that it had been proved that the presence of sodium ions was an essential condition for the physiological activity of both muscular and nervous tissues, and, in the second place, it had been shown that both sodium and calcium ions were essential for the proper action of nervous interconnections, thus tending to prove that some third substance intervened between the two units, *i.e.* that there was discontinuity.

Dr. W. MacDougall argued that the fact that motor neurones could not conduct backwards was the best evidence of discontinuity. Upon the same hypothesis depended also the simplest explanation of another typical characteristic of nervous activity—the effect of summation of weak stimuli. Moreover, the "law of nerve habit" was most difficult to explain except on the assumption that there is some intermediate structure between successive nerve elements which offers a resistance to the transmission of impulses, a block, however, which can be overcome by the action of appropriate stimuli.

Prof. Langley, in replying on the whole discussion, suggested among other points that the strands of material described by Dr. Kerr in the development of nerve fibres might be simply connecting structures along which the nerve fibrillæ, *i.e.* the true nerve element, grew down from the developing neuroblast.

Prof. E. A. Schäfer demonstrated a method of artificial respiration which is a modification of one first suggested by Dr. B. Howard in 1869. In Howard's method the patient in a case of drowning is first turned downwards and the back is pressed on two or three times to force out water from the lungs, after which he is turned face upwards. The operator is then directed to grasp the lower part of the chest and to press gradually forward with all his weight for three seconds, then with a push to jerk himself back and wait three seconds, repeating this eight to ten times a minute.

This method is simple, can be performed by one person, and is fairly efficient so far as air exchange is concerned. The drawbacks are that the tongue in the face-up position tends to fall back and block the passage of air through the pharynx; that there is risk of rupturing the liver; and that there is risk of breaking the ribs if the operator is heavy and powerful and if the patient is advanced in years.

These drawbacks are avoided if the patient be turned into the prone position during the whole procedure. Greater efficiency is thereby attained, and the risk of injury to ribs or viscera is reduced to a minimum. The muscular exertion required is only that needed to swing the upper part of the body backwards and forwards on the hands about twelve or thirteen times a minute, the operator kneeling by the side of or across the patient. The pressure is gradually applied and gradually released. The amount of air exchanged by this method per minute is greater than that yielded by any other which has been tried, and may even exceed the ordinary rate of exchange of the individual.

Tables showing the amount of air exchanged in the various methods which have been recommended for artificial respiration were shown, from which it was seen that only in Howard's method and in Schäfer's modification of that method did the amount reach that attained in normal respiration. Schäfer's method is easy to carry out, even for prolonged periods, and is sufficient for the needs of a normal individual who submits himself to be respired in this manner. On the other hand, in both the Sylvester and Marshall Ward methods, a normal individual is unable to refrain from himself actively respiring on account of the air-exchange being insufficient.

Dr. F. W. Edridge-Green read a paper on the necessity of a lantern test as the official test for colour blindness. Dr. Edridge-Green described two cases, both naval lieutenants, which he had examined, in both of which the men passed the wool test but failed when examined by the lantern test. These were selected because both had daily experience with coloured lights and not with wools. He concluded, then, that because a man can sort wools correctly it does not follow that he can distinguish between coloured lights. In his opinion many varieties of colour blindness may escape detection by the wool test.

On Tuesday, August 23, Prof. A. Kossel (Heidelberg) communicated the main results obtained in work conducted in conjunction with Mr. H. D. Dakin on the protamines. These are the simplest type of proteids. One of them, salmin, when treated with boiling dilute mineral acid yields only five atomic groupings, viz. urea, diamido-valerianic acid, serine, monoamido-valerianic acid, and pyrollidincarboxylic acid. They had investigated the relative proportions in which these five substances were present in salmin, and had found them approximately as follows:—ten molecules of diamido-valerianic acid, ten of urea, two of serine, one molecule of monoamido-valerianic acid, and two molecules of pyrollidincarboxylic acid. The composition of clupein was found to be complicated by the presence of alanine in addition. On the other hand, scrombine possesses an even simpler constitution, for, in addition to urea and diamido-valerianic acid, only alanine and pyrollidincarboxylic acid were found. Sturin obtained from the testes of the sturgeon presents a different constitution. Two diamido-acids are present, diamido-valerianic acid and diamido-caproic acid, the former being combined with urea. To this complex a heterocyclic group, histidine, remains to be added.

The ordinary proteids differ mainly from the protamines in an increased proportion of monoamido-acids, so that the complexity of molecule is extraordinarily great. This complexity is further increased by the addition of other groups, *e.g.* dibasic acids such as aspartic and glutamic acids, which are not present in the protamines.

Prof. J. E. Johansson (Stockholm) gave an account of his experiments upon the immediate effect of carbohydrates upon metabolism. These experiments dealt with the rate of excretion of carbonic acid following the administration of various carbohydrates, and were conducted upon man in a respiration chamber. He first showed that for a particular individual the rate of excretion was practically constant if taken some hours after a meal, and that this rate did not vary with differences in the previous diet nor at different periods of the year. If, then, an individual is given a quantity of a particular carbohydrate about eight hours after a meal, the amount of increase in CO_2 excreted is to be assigned to the food given. He showed in this way that an increase of CO_2 followed the administration of glucose, saccharose, or levulose, and that this increase, which amounted on the whole to from 8 per cent. to 20 per cent. of the total carbon given, began within the first half hour and lasted from two to three hours. The increase persisted longer after saccharose or levulose than after dextrose, and the total amount was greater. He further showed that the amount of the CO_2 surplus was in proportion to the amount of carbohydrate given if this did not exceed 150 grams. The effect of a dose of sugar was greatly influenced by the previous state of nutrition of the person experimented upon. Thus, after a fasting period of forty hours, the amount of carbon retained was much greater than after a ten hours' period. A further point of interest was that the amount and rate of destruction of the various sugars were not influenced by the performance of work. The two effects were additive, and did not interfere with one another.

Mr. P. P. Laidlaw gave the results of some observations on blood pigments. The iron in hæmochromogen is unstable to dilute acids, a fact which shows that hæmatoporphyrin is present in the hæmatin molecule. This was absolutely proved by the artificial formation of hæmochromogen by warming iron free hæmatoporphyrin in an ammoniacal solution to which Stokes's fluid was added from time to time, when on repeatedly reducing the mixture for an hour or so hæmochromogen was formed. The method of synthesis renders it probable that hæmatin is a combination of two hæmatoporphyrin groups with one of iron. Turacin also may be synthesised from hæmatoporphyrin by boiling with cuprammonium solutions.

Dr. F. G. Hopkins wished to lay stress on a further point which had not, perhaps, been brought out very clearly in Mr. Laidlaw's paper. It had been found that if reduced hæmoglobin was treated with a mineral acid, the decomposition went as far as the production of hæmatoporphyrin, whereas, when the same hæmoglobin was converted into oxyhæmoglobin and then treated with acid, hæmatin only was produced. From this it was clear that a very fundamental part was played by the iron in the conversion of hæmoglobin into oxyhæmoglobin.

Prof. A. B. Macallum (Toronto) read a paper on the distribution of potassium in animal and vegetable cells. A solution of the double nitrite of sodium and cobalt gives a yellow precipitate with a potassium salt. If, therefore, thin pieces of the tissue to be examined be treated with this solution, and the excess of the reagent be washed away with ice-cold water, the position of the potassium in the cells is indicated by the presence of the yellow precipitate. Its localisation may be rendered more obvious by converting the yellow precipitate into a black one by further treatment with ammonium sulphide, which precipitates the cobalt as a black sulphide. In this way is was found that potassium is never diffused through the cell. It is more abundant in vegetable than in animal protoplasm, and the nucleus is always absolutely free from it. Only one tissue element was found absolutely free from it, viz. the nerve cell and the axis cylinder. All dead and inert material in a living tissue becomes charged with potassium. This is especially the case with intercellular material.

Dr. W. B. Hardy remarked on the difficulties of determining the distribution of a soluble substance in so small a structure as a cell, for diffusion may be relatively rapid, and thus the results obtained might be very misleading.

Prof. Brodie suggested that the reason why the nucleus never contained any of the precipitate could be readily explained on the assumption that the nuclear membrane was impermeable to the cobalt salt. This, too, would explain the fact pointed out by Prof. Macallum that in many instances the precipitate was found accumulated immediately around the nucleus, for if the nuclear membrane were impermeable to the cobalt salt but permeable to a potassium salt, the latter as it diffused out would be precipitated at once by the excess of cobalt salt present in the surrounding protoplasm.

Prof. W. O. Atwater described his investigations on the nutrition of man at a joint meeting with the section of economics. An abstract of the paper will appear in another issue of NATURE.

Dr. W. Page May brought forward a communication by Prof. Elliot Smith and himself on the motor localisation in the lemur. He showed the area stimulation of which produced movements on the opposite side of the body, and demonstrated that the sequence of representation of movement was in agreement with that of Sherrington and Grünbaum on the ape. It also cleared up the discussion on the homologies of a small sulcus which had previously been described as postcentral and precentral, but which is really central, as Elliot Smith, arguing merely from morphology, pointed out two years ago.

He also described the results of localisation in the dog obtained by Elliot Smith and Wilson, who have shown that the excitable area is limited anteriorly by the crucial sulcus. This result was in harmony with the histological results of Cushing.

In a second communication Dr. Page May discussed the results of previous workers on the optic thalamus, and described some experiments he had made on this subject. He showed that following lesions in the thalamus certain motor disturbances were produced, and that descending paths could be traced from the thalamus into the anterior and lateral columns of the spinal cord. He also showed photographs and specimens of a descending tract hitherto undescribed in the posterior columns of the cervical and dorsal cord. This extended downwards from the thalamic region, and occupied a position near the middle line at the anterior end of Goll's column. In rare cases fibres could be traced into the lowest portions of the spinal cord.

Prof. G. S. Woodhead communicated the results of an investigation on joint-ill in the foal. This is an affection of especial importance to horse-breeders, in which, in addition to certain constitutional symptoms, marked stiffness and swelling makes its appearance in the joints, while at a later stage abscesses form. Investigation of the cause of this disease proved it to be due to a microorganism which gained admittance into the young animal through the cut end of the umbilical cord. From the practical point of view it was therefore evident that such precautions as are taken against septic infection in the case of the child at birth should also be taken in the case of the foal.

Dr. T. S. P. Strangeways gave an account of a committee of pathological research which is being founded with the object of investigating some of the more important diseases the pathology of which is as yet undetermined. The proposed lines on which the committee intends to act is to select some special disease and make an exhaustive study of it from all sides, a study which will last for two to three years. It is proposed to found a small hospital which shall be devoted entirely to cases of that disease during its period of study. The committee is to be a comprehensive one, and include all who will watch the course of the disease or who will undertake research work on the subject. These will report to a central body, which will also be responsible for the distribution of the collected facts and literature of the subject to those actively engaged in working upon it.

Prof. C. S. Sherrington and Miss S. C. M. Sowton communicated the results of an investigation into the amount of chloroform which, when administered to the heart, can dangerously embarrass its action. For this inquiry they had adopted the method, gradually evolved of recent years, of keeping the excised heart of a mammal alive by perfusing its blood-vessels with warm nutrient solutions. The heart used by them was that of the cat. The beating of auricle and ventricle was recorded graphically. The effect of chloroform was examined by allowing the perfusing fluid, pure saline, serum, or blood, as the case might be, to be replaced by a similar fluid to which chloroform in known quantity had been added. When this was done the chloroform showed its effect, practically at once, by diminishing the amplitude of the beat without altering its rate. The amount of the diminution was proportionate, within limits, to the concentration of the solution of chloroform. When exhibited in saline solution, chloroform showed a depressant action even in a dilution of 1 part in 150,000 of the saline solution. The full amount of the depression caused by a given solution was rapidly reached, *e.g.* in a minute, and then the continued administration of that solution caused no further depression—even if continued for half an hour at a time. That is to say, there is no cumulative action of the drug detectable in the isolated heart so perfused for a period of half an hour. On the contrary, there was generally evidence of a slight waning of the depression as the exhibition of the drug was uninterruptedly maintained. This tolerance was, however, quite evanescent, for on interrupting the perfusion with the chloroform solution and then returning to it, the depression recurred in its original depth. On discontinuing the perfusion with chloroform solution and reverting to the chloroform-free fluid, the depression caused by the chloroform—unless the chloroform solution has been of great concentration—is extremely rapidly removed, even when the beat of the heart has been for many minutes practically abolished. This suggests the view that the effect of chloroform on the cardiac muscle is due to the formation of some easily dissociable compound between the chloroform and some active constituent of the tissue. It has been recently urged by Moore and Roaf that this constituent is a proteid, and in favour of this view is a further fact elicited in the present inquiry. On comparing the amount of depression of a chloroform solution of given concentration, in salt solution on the one hand and in blood on the other, it is found that the effect of that concentration in blood is much less than it is in salt solution. In other words, the effect of a chloroform solution of given concentration in blood is only equivalent to that of a solution barely one-twelfth as concentrated in salt solution. This can be explained by supposing that the salt solution, though it supports the beat of the heart, supports it less well than does blood; but the more important part of the explanation seems to be that the tension of the chloroform in the blood is much less than in the salt solution. In other words, the difference seems referable to some constituent of the blood taking up and holding, in a relatively inactive form, a considerable fraction of the chloroform added to it. The chloroform added distributes itself in that complex fluid according to a coefficient of partition. It is only what is left over freely dissolved in it which is available for acting on the heart tissue. Comparative estimations of the depressant effect in blood, serum, and saline solution show that serum is intermediate between the other two, so that evidently the corpuscles contain, in large measure, a substance that combines with the chloroform.

THE RELATION OF OXIDATION TO FUNCTIONAL ACTIVITY.[1]

IN opening this discussion, Sir John Burdon-Sanderson said :—

In undertaking to open this discussion, I do not claim to contribute any results of my own researches or to speak on any subject *ex cathedra*, or with any degree of finality. I propose to state very shortly what seem to me the discussable questions, *i.e.* those respecting which we have experimental data, and to submit to the section those on which we need enlightenment.

The title is "Oxidation and Functional Activity." May I say that, without criticising it, I would ask for some latitude as regards the word oxidation. By oxidation is meant the formation of an oxide. Now we know that in the living organism oxygen may and does act without this happening, *e.g.* in those processes of which the oxygenating of the colouring matter of the blood is the type.

This is so important a distinction that I would suggest to substitute in these cases the term "oxygenation." The subject of our discussion would then be rightly stated as follows :—"The Relation between Oxygen and the Chemical Processes which Constitute Animal and Plant Life." The older notion of the part played by oxygen in the chemical processes of life was that it was a destroyer and not a maintainer of the chemical energies of the cell. We now recognise that oxygen may have a double function to perform, first as an element the presence of which is essential to the anabolic process by which living matter is built up, and secondly as equally essential to the disintegrative process which, taking muscular activity as the type of others, is associated with the performance of function. Of these two actions, in each of which oxygen is concerned—the *con*structive and the *de*structive—the second is better under-

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[1] Report of a discussion before the Section of Physiology at the Cambridge meeting of the British Association, August 19.